



# Descriptive Statistics for Modern Test Score Distributions: Skewness, Kurtosis, Discreteness, and Ceiling Effects

## Citation

Ho, A. D., and C. C. Yu. 2014. Descriptive Statistics for Modern Test Score Distributions: Skewness, Kurtosis, Discreteness, and Ceiling Effects. *Educational and Psychological Measurement* 75 (3) (September 15): 365–388.

## Published Version

doi:10.1177/0013164414548576

## Permanent link

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**Descriptive Statistics for Modern Test Score Distributions:  
Skewness, Kurtosis, Discreteness, and Ceiling Effects**

Andrew D. Ho and Carol C. Yu

*Harvard Graduate School of Education*

Author Note

Andrew D. Ho is an Associate Professor at the Harvard Graduate School of Education, 455 Gutman Library, 6 Appian Way, Cambridge, MA 02138; email: [Andrew\\_Ho@gse.harvard.edu](mailto:Andrew_Ho@gse.harvard.edu)

Carol C. Yu is a Research Associate at the Harvard Graduate School of Education, 407 Larsen Hall, 14 Appian Way, Cambridge, MA 02138; email: [Carol\\_Yu@gse.harvard.edu](mailto:Carol_Yu@gse.harvard.edu)

This research was supported by a grant from the Institute of Education Sciences (R305D110018). The opinions expressed are ours and do not represent views of the Institute or the U.S. Department of Education. We claim responsibility for any errors.

## Descriptive Statistics for Modern Test Score Distributions:

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**Abstract**

Many statistical analyses benefit from the assumption that unconditional or conditional distributions are continuous and normal. Over fifty years ago in this journal, Lord (1955) and Cook (1959) chronicled departures from normality in educational tests, and Micceri (1989) similarly showed that the normality assumption is met rarely in educational and psychological practice. In this paper, the authors extend these previous analyses to state-level educational test score distributions that are an increasingly common target of high-stakes analysis and interpretation. Among 504 scale-score and raw-score distributions from state testing programs from recent years, non-normal distributions are common and are often associated with particular state programs. The authors explain how scaling procedures from Item Response Theory lead to non-normal distributions as well as unusual patterns of discreteness. The authors recommend that distributional descriptive statistics be calculated routinely to inform model selection for large-scale test score data, providing warnings in the form of sensitivity studies that compare baseline results to those from normalized score scales.

## Descriptive Statistics for Modern Test Score Distributions:

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**Introduction**

Normality is a useful assumption in many modeling frameworks, including the general linear model, which is well known to assume normally distributed residuals, and structural equation modeling, where normal-theory-based maximum likelihood estimation is a common starting point (e.g., Bollen, 1989). There is a vast literature that describes consequences of violating normality assumptions in various modeling frameworks and for their associated statistical tests. A similarly substantial literature has introduced alternative frameworks and tests that are robust or invariant to violations of normality assumptions. A classic, constrained example of such a topic is the sensitivity of the independent-samples t-test to normality assumptions (e.g., Boneau, 1960), where violations of normality may motivate a robust or nonparametric alternative (e.g., Mann & Whitney, 1947).

An essential assumption that underlies this kind of research is that the degree of non-normality in real-world distributions is sufficient to threaten the desired interpretation in which the researcher is most interested. If most distributions in a particular area of application are normal, then illustrating consequences of non-normality and motivating alternative frameworks may be interesting theoretically but of limited practical importance. To discount this possibility, researchers generally include a real-world example of non-normal data, or they at least simulate data from non-normal distributions that share features with real-world data. Nonetheless, comprehensive reviews of the non-normality of data in educational and psychological applications are rare. Almost sixty years ago in this journal, Lord (1955) reviewed the skewness and kurtosis of 48 aptitude, admissions, and certification tests. He found that test score distributions were generally negatively skewed and platykurtic. Cook (1959) replicated Lord's analysis with 50 classroom tests. Micceri (1989) gathered 440 distributions, 176 of these from large-scale educational tests, and he described 29% of the 440 as moderately asymmetric and 31% of the 440 as extremely asymmetric. He also observed that all 440 of his distributions were non-normal as indicated by repeated application of the Kolmogorov-Smirnov test ( $p < .01$ ).

In this paper, we provide the first review that we have found of the descriptive features of state-level educational test score distributions. We are motivated by the increasing use of these data for both research and high-stakes inferences about students, teachers, administrators, schools, and policies. These data are often stored in longitudinal data structures (e.g., U.S. Department of Education, 2011) that laudably lower the barriers to the analysis of educational test score data. However, as we demonstrate, these distributions have features that can threaten conventional analyses and interpretations therefrom, and casual application of familiar parametric models may lead to unwarranted inferences.

Such a statement is necessarily conditional on the model and the desired inferences. We take the Miccerri (1989) finding for granted in our data: these distributions are not normal. At our state-level sample sizes, we can easily reject the null hypothesis that distributions are normal, but this is hardly surprising. The important questions concern the magnitude of non-normality and the consequences for particular models and inferences. We address the question of magnitude in depth by presenting skewness, kurtosis, and discreteness indices for 504 raw and scale score distributions from state testing programs. Skewness and kurtosis are well established descriptive statistics for distributions (Pearson, 1895) and are occasionally used as benchmarks for non-normality (e.g., Bulmer, 1979). We illustrate the consequences of non-normality only partially. This is deliberate. A complete review of all possible analyses and consequences is impossible given space restrictions. Thus, our primary goal is to make the basic features of test score distributions easily describable and widely known. These features may guide simulation studies for future investigations of the consequences of violating model assumptions. Additionally, if variability in these features is considerable, this motivates researchers to use an arsenal of diverse methods to achieve their aims, with which they might manage tradeoffs between Type I and Type II errors, as well as bias and efficiency.

We have two secondary goals. First, we provide illustrative examples of how these features can lead to consequential differences for model results, so that researchers fitting their own models to these data may better anticipate whether problems may arise. Second, we explain the pathology of the non-normality that we observe in the data. We demonstrate that, when test score distributions are cast as the

results of particular models and scaling procedures, their non-normal features should not be surprising. We emphasize that non-normality is not inherently incorrect or flawed—it is the responsibility of the researcher to fit the model to the data, not the reverse. However, if the resulting features are undesirable for the primary intended use of the test scores, then the procedures that generate the distributions should be reconsidered.

To accomplish these multiple goals, we present this paper in four parts. The first part introduces the pathology of non-normality by describing skewness and kurtosis of raw score distributions as natural properties of the binomial and beta-binomial distributions. This analysis of raw scores serves as a conceptual link to the Lord (1955), Cook (1959), and Micceri (1989) analyses, which were dominantly raw-score-based, and sets an historical baseline from which to evaluate modern uses of test scores, which are scale-score-based, that is, using scores developed using a particular scaling method. In all of the cases presented here, this scaling method is Item Response Theory (IRT; see Lord, 1980). In the second part, we present the primary results of the paper in terms of skewness and kurtosis of both raw score and scale score distributions. We continue to emphasize the pathology of non-normality by describing the differences between features of raw score and scale score distributions as a consequence of scaling methods using IRT.

The third part of the paper uses visual inspection of test score distributions to motivate additional descriptive statistics for scale score distributions. In particular, we motivate statistics that describe the discreteness of distributions, and we show that visually obvious “ceiling effects” in some distributions are better identified by discreteness than by skewness. Here, we begin to illustrate simple consequences that threaten common uses of test scores, including comparing student scores, selecting students into programs, and evaluating student growth. Finally, in the fourth part of the paper, we describe the possible impact of non-normality on the results of regression-based test score analyses. We illustrate this with results from predictive models for students and a “value-added”-type model for school-level scores. For these sensitivity studies, we compare results estimated using observed distributions to results estimated using their normalized counterparts. This answers the question, if these distributions were normal, how

would our interpretations differ? Taken together, these arguments build toward a familiar if important principle: researchers should select models with knowledge of the process that generated the data and informed by data features, or else risk flawed analyses, interpretations, and decisions.

### **Data**

A search of publicly available state-level test-score distributions yielded 330 scale score distributions and 174 raw score distributions from 14 different state testing programs in the academic years ending 2010 and 2011. We constrained the time period to these two years because these state testing programs were fairly stable at that time and because all states had some data in both years. One of the programs, the New England Common Assessment Program (NECAP), represents 4 states, Maine, New Hampshire, Rhode Island, and Vermont, thus the data represent 17 states in total. As Micerri (1989) noted, the data appropriate for full distributional analyses are rarely publicly available, however, these states have considerable regional coverage across the United States. We collected distributions from 6 grades (3-8) and 2 subjects (Reading/English Language Arts and Mathematics), for a total of 12 possible scale score distributions per year.

Raw score distributions were available from 8 of the 14 state testing programs. We use raw scores largely for illustration, as a historical reference point to previous research, and as a conceptual reference point to emphasize the consequences of IRT scaling on skewness and kurtosis. Since scores are generally reported and analyzed using scale scores, the latter half of the paper focuses on scale scores. Nebraska is missing Mathematics score distributions in 2010, and Oklahoma is missing raw score distributions in 2010.

Table 1 shows the minimum and maximum number of examinees across the available score distributions in each state. Unsurprisingly, these numbers are indicators of the relative youth populations of each state. The distribution with the highest n-count is from Texas with 689938 examinees, and the distribution with the lowest n-count is from South Dakota with 8982 examinees. Altogether, the distributions represent data from over 31 million examinees. In the last two columns, Table 1 shows the minimum and maximum number of discrete score points in the scale score distributions. Distributions

from Colorado are outliers in terms of their counts of discrete score points, with counts around 500 due to their practice of scoring based on patterns of item responses rather than summed scores (CTB-McGraw Hill, 2010a, 2011a). Distributions from all other states have discrete score point counts ranging from 28 (New York, Grade 5, English Language Arts, in 2010) to 93 (Texas, Grade 7, Reading, in 2011).

### Skewness and Kurtosis

We use skewness and kurtosis as rough indicators of the degree of normality of distributions or the lack thereof. Unlike test statistics from normality testing procedures like the Kolmogorov-Smirnov  $D$  or the Shapiro-Wilk  $W$ , skewness and kurtosis are used here like an effect size, to communicate the degree of non-normality, rather than statistical significance under some null hypothesis of normality. The use of skewness and kurtosis to describe distributions dates back to Pearson (1895) and has been reviewed more recently by Moors (1986), D’Agostino, Belanger, and D’Agostino (1990), and DeCarlo (1997). Skewness is a rough index of the asymmetry of a distribution, where positive skewness in unimodal distributions suggests relatively plentiful and/or extreme positive values, and negative skewness suggests the same for negative values. Skewness is an estimate of the third standardized moment of the population distribution,

$$s = \frac{\sqrt{n(n-1)}}{n-2} \frac{\frac{1}{n} \sum_i (x_i - \bar{x})^3}{\left(\frac{1}{n} \sum_i (x_i - \bar{x})^2\right)^{3/2}}. \quad (1)$$

Skewness can range from  $-\infty$  to  $+\infty$ , and symmetric distributions like the normal distribution have a skewness of 0. The  $n$ -based bias correction term for the population estimate is negligible for the large samples that we have here, but we include it for completeness.

Kurtosis is an estimate of the fourth standardized moment of the population distribution,

$$k = \frac{n(n+1)(n-1)}{(n-2)(n-3)} \frac{\sum_i (x_i - \bar{x})^4}{(\sum_i (x_i - \bar{x})^2)^2}. \quad (2)$$

Kurtosis can range from 1 to  $+\infty$ . The kurtosis of a normal distribution is 3. Although it is common to subtract 3 from  $k$  and describe this as “excess kurtosis”—beyond that expected from a normal



distribution—we use the definition above, where  $k < 3$  is platykurtic (less peakedness, weaker “tails,” heavy “shoulders”), and  $k > 3$  is leptokurtic (more peakedness, heavy tails, weak shoulders). As references, a uniform distribution has a kurtosis of 1.8 (platykurtic), and a logistic distribution has a kurtosis of 4.2 (leptokurtic).

### Part 1: Raw Score Distributions

In this section, we begin to demonstrate that the non-normal features of test score distributions are the byproducts of well-established models and scaling procedures. Lord (1955) and Cook (1959) report two findings: (1) raw score distributions from “easy” tests generally have negative skew, and (2) symmetric raw score distributions tend to be platykurtic (kurtosis  $< 3$ )<sup>1</sup>. They both operationalized the “easiness” of a test as the mean raw score over the maximum possible score. This is equivalent to the average of item proportions correct,  $\bar{p}$ , where “easy” tests have  $\bar{p} > 0.5$ . Figure 1 replicates their findings empirically. The skewness and kurtosis of raw score distributions are plotted on a scatterplot in light gray and are concentrated in the lower left of the plot. All 174 raw score distributions meet the Lord-Cook definition of “easy,” where the minimum  $\bar{p}$  across 174 distributions is 55.41% (not shown). Figure 1 shows that all raw score distributions, in light gray, have negative skew. In addition, Figure 1 shows that the raw score distributions that are closest to 0 skew are all platykurtic, with kurtosis  $< 3$ .

These findings are straightforward to explain in terms of models for test score distributions that have become well established since the publication of the Lord (1955) and Cook (1959) reviews. Even from a naïve model for test scores that assumes all item difficulties are the same (all  $p_i = p$ ), we can demonstrate that easy tests will have negatively skewed score distributions using a binomial model, which has a known skewness of,

$$s_{binomial} = \frac{1 - 2p}{\sqrt{ip(1 - p)}}$$

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<sup>1</sup> Both Lord(1955) and Cook (1959) operationalized skewness and kurtosis using percentile-based statistics (e.g., skewness as  $(P_{90} + P_{10})/2 - P_{50}$ ). We use the modern definitions of skewness and kurtosis specified in Equations 1 and 2. We also replicated results using the percentile-based definitions of skewness and kurtosis, and they do not change any substantive conclusions.

Here,  $p$  is the common item difficulty, and  $i$  is the number of items on the test. For any  $p > .5$ , the skewness will be negative, consistent with the first Lord-Cook finding.

Given that item proportions correct  $p_i$  are not constant across items in practice, a more appropriate model for raw score distributions is the beta-binomial distribution, where the distribution of item proportions correct  $p_i$  is modeled by a 2-parameter beta distribution. The beta distribution for  $p_i$  is defined by parameters  $\alpha$  and  $\beta$ , where the mean of the distribution of  $p_i$  will be  $\mu_p = \frac{\alpha}{\alpha + \beta}$ .

We can use the beta-binomial model to explain the second Lord-Cook finding: symmetric raw score distributions tend to be platykurtic. To illustrate this, we constrain the  $\alpha$  and  $\beta$  parameters that control the beta distribution of  $p$  values to sum to 4. This constrains  $\alpha$  and  $\beta$  for any chosen  $\mu_p$ , for example, if  $\mu_p = 0.75$ , then  $\alpha = 3$  and  $\beta = 1$ . We then assume a 50-item test, consistent with a central number of discrete score points from Table 1.

Next, given the known expressions for the skewness and kurtosis of the beta-binomial distribution (these expressions are complex and add little insight, so we defer to statistical reference texts), we can establish a theoretical relationship between skewness and kurtosis under these constraints. This relationship is shown as a dashed line in Figure 1 for various levels of  $\mu_p$ . As Figure 1 shows, a symmetric distribution (modeled as beta-binomial when  $\mu_p = 0.5$  and  $\alpha = \beta = 2$ ) is platykurtic with kurtosis  $< 3$ . Similarly, the location of raw score distributions on and around the dashed line in Figure 1 shows that the skewness and kurtosis of raw score distributions is well explained by a beta-binomial model parameterized by  $\mu_p$ . Easier tests with higher  $\mu_p$  will be negatively skewed. Symmetric tests will be platykurtic. And harder tests with lower  $\mu_p$  will be positively skewed.

Although alternative constraints (for  $\alpha$  and  $\beta$  in terms of  $\mu_p$ ) and more flexible models for test scores (like the 4-parameter beta-binomial model; Hanson, 1991) are available, this illustration suffices to demonstrate that the Lord-Cook skewness and kurtosis findings are consistent with the known behavior of well-established models for raw score distributions. Raw score distributions will be non-normal to the degree that they are easier or harder for their respective populations. They will also become progressively

more leptokurtic as the average difficulty ranges away from 50%. If raw score distributions are the target of analysis and model results are known to be sensitive to skewness, for example, concerns will arise only if the test is quite easy or quite hard for its respective population.

## **Part 2: Skewness and Kurtosis of Scale Score Distributions**

In this section, we transition our focus from indices of non-normality of raw score distributions to indices of non-normality of scale score distributions. The theoretical and practical benefits of modern scaling methods, most notably those deriving from Item Response Theory (Lord, 1980; Yen & Fitzpatrick, 2006), are substantial, and raw scores are no longer the basis for most large-scale test score scaling and reporting. This motivates a review of scale score distributions, which are shown in black in Figure 1. Although about half of scale score distributions do not have a raw score counterpart, we can see that the effect of IRT scaling on skewness and kurtosis is generally a balancing of skewness and an increase in kurtosis. We explain the pathology of this effect later in this section.

Table 2, which overviews median, minimum, and maximum skewness and kurtosis of distributions for each state, also reflects this tendency, as states that have both raw and scale scores show median skewness generally balancing toward 0 from raw to scale scores, and median kurtosis generally increasing from raw to scale scores. Across all states, median skewness for raw scores is negative and median kurtosis for raw scores is less than 3, and median skewness for scale scores is negligibly positive, and median kurtosis for scale scores is greater than 3. New York is separated by academic year, because, unlike other states, the distributions of skewness and kurtosis differed substantially across years. Figure 2 also shows box plots representing distributions of skewness and kurtosis by state. Note that there is a very strong state effect on skewness and kurtosis indices; these distributional features are very similar within states—even across grades and subjects—and very different among states.

Table 2 and Figure 2 show that Colorado and New York are outlying states in terms of skewness and kurtosis, with median distributions in Colorado and New York in 2011 showing considerable negative skew, and median distributions in New York in 2010 showing considerable positive skew. Both show heavily leptokurtic distributions. The most negatively skewed Colorado distributions happen to be

from the Reading tests and are more heavily skewed in lower grades. In New York, most of the skewed distributions happen to be from the reading test, as well.

Here, we continue with an effort to describe the pathology of the non-normal features of score distributions, in this case, scale score distributions. State-by-state explanation of the reasons for particular levels of skewness and kurtosis is beyond the scope of this paper. Such explanation would require item-level data and/or particular knowledge of scaling practices that are not always available in public technical reports. As we noted earlier, there is also nothing inherently incorrect or flawed about score distributions with extreme levels of skewness and kurtosis, as long as they arise from a defensible scaling and linking procedure that supports accurate user interpretations of scores and successive differences between scores. Nonetheless, we review three factors that may help to explain how these extreme values of skewness and kurtosis arise operationally.

First, most operational scoring procedures produce a 1-to-1 mapping of summed “number correct” scores to scale scores (Yen & Fitzpatrick, 2006). These mappings align with student and teacher intuition about the scoring of classroom tests but may sacrifice useful information deriving from patterns of item responses. Some 1-to-1 mappings derive naturally from simple IRT models like the 1-parameter logistic model. Other scoring methods, like “inverted test characteristic curve (TCC)” approaches (Yen, 1984), operate similarly to a logit transformation of percentage-correct scores,<sup>2</sup> such that high and low scores are stretched to extremes. This naturally increases kurtosis, consistent with the differences between raw-score and scale-score kurtosis shown in Figure 1 and Table 2. When there are disproportionate frequencies at high or low scores, these transformations may result in positive or negative skew, respectively. This procedure was used in New York and represents an explanation for the considerable skewness and kurtosis of New York score distributions.

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<sup>2</sup> Lord (1980) showed that, for 1- and 2-parameter logistic IRT models, maximum likelihood estimates of proficiency parameters are in fact a linear function of logit-transformed percentage-correct scores, in the special case when items are functionally equivalent.

Second, many scoring approaches have undefined perfect and zero scores (in the case of maximum likelihood estimation in IRT), and others also have undefined scores below chance (in the case of inverted TCC approaches). In these cases, a lowest and highest obtainable scale score (LOSS and HOSS, respectively) is determined judgmentally or by some established extrapolation procedure (e.g., Kolen & Brennan, 2002). The selection and location of these scores can affect skewness and kurtosis, especially when large percentages of examinees obtain perfect scores or scores below chance. In general, when they are extreme compared to the rest of the distribution, kurtosis will increase, and distributions will be relatively skewed when densities are imbalanced toward either LOSS or HOSS. When HOSS and LOSS are forced to be equal over many years or across multiple grades, the linking constraints can draw distributions farther from HOSS and/or LOSS, and this would also affect skewness and kurtosis. We return to this topic in the next section.

Third, certain year-to-year and grade-to-grade linking approaches may affect skewness and kurtosis. Linking that relies on linear transformations, such as mean-sigma linking and characteristic curve linking (e.g., Kolen & Brennan, 2002), will not affect skewness and kurtosis any more than the separate IRT calibrations upon which they are based. However, constrained or concurrent calibration (e.g., Yen & Fitzpatrick, 2006), where parameters of item characteristic curves are constrained to or simultaneously estimated with those from previous years or different grades, may lead to distributions with extreme skewness and kurtosis. This may manifest in distributions that are systematically more skewed across grade levels or over time. Incidentally, this is the pattern that arises in Colorado, where skewness is greater toward the lower grades (not shown).

Our intent in reviewing these procedures is to explain why some score distributions in some states may exhibit different or particularly extreme skewness and kurtosis values. Again, we emphasize that non-normality is a feature and not necessarily a flaw. Test designers may reconsider the use of particular scoring procedures if this degree of non-normality threatens intended test score uses. For secondary analysts, any resulting non-normality must be handled with the usual iterative model fitting and

diagnostic process, with the use of sensitivity studies where appropriate.

### **Part 3: Discreteness and Ceiling Effects**

The skewness of variables is used not only as an index of non-normality but also as an index for “ceiling effects” or “floor effects,” where positively skewed variables are assumed to have a floor effect, and negatively skewed variables are assumed to have a ceiling effect (e.g., Koedel & Betts, 2010).

Ceiling and floor effects are also assumed whenever data are censored, that is, when the locations of scores above or below a particular ceiling/floor are not known, and they are only known to be at or above/below that score. Whereas non-normality may motivate robust or nonparametric procedures, these censored data motivate tobit regression and other censored data models (Long, 1997; Tobin, 1958).

We define ceiling and floor effects as insufficient measurement precision to support desired distinctions between examinees at the upper and lower regions of the score scale, respectively. In this section, we argue that skewness is neither sufficient nor necessary for ceiling or floor effects. We then present some simple descriptive statistics that highlight unusual features of modern test score distributions that are akin to ceiling and floor effects—features that skewness and kurtosis alone do not capture. Finally, we provide illustrative examples of consequences of this discreteness in common, practical uses of test scores.

We begin by demonstrating that a strongly skewed distribution may nonetheless retain measurement precision in score regions that seem to be compressed. A well-established feature of logistic IRT scales is conditional standard errors of measurement that are smallest for central scores and largest at extremes. This contrasts with conditional standard errors of measurement for raw scores, which are largest for central scores and smallest at extremes (Haertel, 2006). Following Lord (1980), we consider an IRT-based scoring procedure that results in a symmetric distribution on some score scale  $\theta$ . Next, we consider a nonlinear monotonic transformation to an alternative score scale  $\theta^*$ , for example, an exponential transformation that results in a relative compression of low scores and a relative stretching of high scores. This results in a positively skewed distribution and, arguably, a floor effect.

As Lord (1980) demonstrates, however, information at any score point on the scale of  $\theta^*$  is proportional to  $\left(\frac{d\theta^*}{d\theta}\right)^{-2}$ , which will be relatively high where the score scale  $\theta^*$  is compressed compared to  $\theta$ . Although the low end of a score scale may be relatively compressed under the transformation, the conditional standard error will decrease as a result. This demonstrates that compression of a score scale is not necessarily synonymous with a loss of information in the compressed region. In short, a skewed distribution is not sufficient to indicate a ceiling or floor effect.

In Figure 3, we also demonstrate that skewed distributions are not necessary for ceiling or floor effects. To construct Figure 3, we listed all distributions with skewness between  $\pm 0.1$  and kurtosis between 2.75 and 3.75, that is, fairly symmetrical distributions with little excess kurtosis. Of these 46 distributions, we selected 6 illustrative discrete histograms of scale score distributions from a range of states, grades, and subjects. Figure 3 shows that all 6 of these distributions show one interesting feature: extremely sparse score points toward the positive extremes of the score scale. Notably, these distributions do not appear symmetric at a glance. This is something of an optical illusion, where it is difficult to reconcile the coarseness and fineness of the spacing between adjacent score points, on the one hand, with the percentage of examinees at each score point on the other.

Although this is a purposeful sample, these are not outlying distributions; many additional symmetric (and asymmetric) distributions also show this feature. We use these distributions to illustrate that it is often discreteness at the positive ends of the score scale—in particular, a small number of score points distinguishing between large percentages of examinees—that can raise concerns about ceiling effects, even in cases where distributions are symmetric, as measured by skewness, and do not show excess kurtosis. If test score users wish to distinguish among the highest 5-10% of high-scoring examinees in these distributions, such distinctions would be difficult or impossible to justify. This demonstrates that skewness is not necessary for ceiling effects. These features are more consistent with a definition of ceiling effects that relates to censored data.

Figure 4 provides descriptive statistics that further illustrate the sparseness of discrete score points at the positive end of the score scale. The top tile of the figure shows the actual count of discrete score points that distinguish among the top 10% of examinees. For example, if more than 10% of examinees are located in the single top score point, then this count is 1. If 5% are in the top score point and 6% are in the second highest score point, then this count is 2. Figure 4 shows that many states have only a few score points distinguishing among the top 10% of examinees. Some distributions in New York and Texas have only 1 score point distinguishing among the top 10% (that is, there is no distinction at all), and the Texas, New York (in 2000), and Idaho median number of distinguishing score points is 2, 3, and 4, respectively.

The middle tile of Figure 4 shows the percentage of total discrete score points that distinguish among the top 10% of examinees. Although it may seem that a small percentage of score points that distinguish in this range is reasonable, it is worth referencing this to what is expected for discrete unimodal distributions. If we define a discrete standard normal distribution as one that divides the range  $[-c, c]$  into evenly spaced bins undergirding a normal distribution, then the expected percentage of discrete score points that distinguish among the bottom or top proportion  $q$  of examinees is given by:  $\frac{\Phi^{-1}(q)+c}{2c}$ , where  $\Phi^{-1}$  is the inverse normal cumulative distribution function. This percentage is 28.64% when the discrete score bins range from -3 to +3 and desired distinctions are among the top 10% ( $c = 3$  and  $q = .1$ ). As Figure 4 shows, the proportion of discrete score bins distinguishing the top 10% is far less than expected under this discrete normal model. On the other side of these distributions, the proportions of discrete score bins distinguishing the bottom 10% are far greater than expected in almost all states (not shown).

The top two tiles of Figure 4 are specific instantiations of a general question: What percentage of examinees is distinguished by what number (or percentage) of discrete scores? Although we chose the top 10%, any other percentage leads to the same conclusion: the higher ends of modern scale score distributions have larger percentages of examinees than those at the lower ends. We consider this to be a



kind of asymmetry that derives from the discrete binning of scores. This asymmetry can arise even when score distributions are not skewed, as Figure 3 demonstrates.

A useful way to conceptualize this is to recognize that, if the scale score points were equally spaced, the distributions would be negatively skewed. For 1-to-1 score mappings, this equally spaced scale is equivalent to the raw score scale. This therefore represents a restatement of the Lord-Cook finding from a Part 1: tests are generally “easy” ( $\bar{p} > 0.5$ ), thus distributions are negatively skewed. The fact that many distributions are nevertheless “symmetric” as determined by skewness reflects that there are two features of these distributions that are in balance. The first is the disproportionate density of examinees in higher score points. The second is the relative stretching of the higher region of the score scale. For the symmetric distributions that we show in Figure 3, these two features offset.

The third tile in Figure 4 highlights this second feature more dramatically, by describing the isolation of the highest score point in terms of standard deviation units. This last tile is akin to answering the question, what is the difference between getting the second highest score and the highest score, in standard deviation units? As Figure 3 already illustrates, the highest score point can often be very isolated. Figure 4 shows that the highest score point is often more than a full standard deviation unit from the second highest score.

As we have stated earlier, our purpose is primarily to describe the features of score distributions and secondarily to illustrate the consequences across the space of possible analyses. Figures 3 and 4 highlight features of score distributions that we do not believe are well appreciated. Whereas score points for raw score distributions are evenly spaced and bounded, scale score distributions are far sparser at their upper ends and occasionally have outlying positive extremes due either to HOSS decisions or chosen IRT scoring procedures. Raw score distributions are generally well summarized by skewness and kurtosis given their evenly spaced score points. In contrast, the symmetry of scale score distributions is a confounded interplay between disproportionate density and uneven spacing between discrete score points. We have provided illustrations and simple descriptive statistics that begin to capture these peculiar features.

We now turn toward the secondary goal of illustrating the consequences of this discreteness, in order to provide cues for others to recognize when their research question or procedure may be threatened by common features of scale score distributions. Three examples of simple, practical consequences follow directly from Figure 3. The first example arises in the context of individual score reporting. The bottom tile of Figure 3 shows that the scale score difference between two students, one with a perfect score and one who answers a single item incorrect, can be greater than 1 standard deviation in 8 states, and greater than 2 standard deviations in 3 states. For students, parents, and teachers, the idea that getting one more item correct would result in the equivalent of, say, 100 to 200 SAT score points, would be at least confusing and very likely controversial.

The second example concerns a common secondary use of educational test scores: selection into special programs. As one example, gifted programs commonly use a percentile-rank-based cutoff as a partial or sole criterion for eligibility, sometimes the 90<sup>th</sup> percentile, and sometimes the 95<sup>th</sup> or 96<sup>th</sup> (e.g., McGinley, Herring, & Zacherl, 2013). If the norm group for these percentiles were the empirical distribution, there are distributions in New York and Texas where these three percentiles would be literally indistinguishable—they would refer to the exact same score, because the highest score point in some of these distributions contains more than 10% of students. If the norm group for these percentiles is a national norm group or any other reference group, then the percentile-rank-based inference may be correct on average, but there would likely be dramatic practical consequences depending upon whether the heavily loaded top score point happened to be located above or below the cutoff.

The third example is an extension of the first that highlights the increasingly common use of educational score scales to support interpretations about student growth. If we interpret the scale underlying the distributions in Figure 3 as a vertical scale capable of supporting repeated measures on the same students, raw score gains in the lower regions of the score scale are dramatically underemphasized on the IRT score scale, and raw score gains in the upper regions are dramatically overemphasized. The fact that different raw score differences map to different scale score differences is hardly noteworthy. However, the degree shown in Figures 3 and 4 is striking and threatens superficial “face” validity. There

may be statistical warrant for a 1-row-score-point gain at the top of the scale equaling a 10-row-score-point gain at the bottom, but rarely is modern test score usage accountable to statistical criteria alone.

#### **Part 4: Consequences of skewness on regression-based analyses.**

This section continues the illustration of consequences by describing the magnitude of the impact of non-normality on common regression-based uses of test scores. The first use considered is simple linear prediction, and the second is the estimation of so-called “value added” effects (McCaffrey, Lockwood, Koretz, Louis, & Hamilton, 2004). To establish a reference for comparison, we normalize score distributions, reducing their skewness and excess kurtosis. By comparing target statistics estimated from observed and normalized data, we obtain the starkest answer to the question: if these data were normal, what difference would it make for this particular analysis and interpretations therefrom?

For this real data example, we use a longitudinal, student-level dataset from a large eastern state. Five grade cohorts have available data across two consecutive years, resulting in five examples of two-timepoint analyses. Note that this analysis requires student-level linkages facilitating longitudinal analysis—these are not publicly available data. The target statistics that signal possible substantive consequences is twofold: the difference between grade-to-grade correlations from observed vs. normalized data, and the correlation between school effects estimated from observed vs. normalized data. School effects are school-level averages of student-level residuals (e.g., Chetty, Friedman, & Rockoff, in press) from a simple linear regression model. We also obtain shrinkage estimates from a model with school random effects (e.g., Rabe-Hesketh & Skrondal, 2012), but the practical difference in terms of our outcomes of interest are inconsequential, and we opt for the simpler metric for its transparency and its straightforward replicability.

Given 2010 ( $x$ ) and 2011 ( $y$ ) scores from student  $i$  and school  $j$ , we estimate separately for each grade:

$$y_{ij} = \beta_0 + \beta_1 x_{ij} + \epsilon_{ij}$$

$$\epsilon_{ij} \sim N(0, \sigma_\epsilon^2)$$

Without loss of generality, we assume  $y_{ij}$  and  $x_{ij}$  are already standardized, thus  $\beta_1 = \rho_{xy}$ , the grade-to-grade correlation of interest, and  $\hat{\beta}^2 = r_{xy}^2 = R^2$ , the proportion of variance in  $y$  accounted for by  $x$ . For school effects, we take student-level residuals,  $e_{ij} = y_{ij} - \hat{y}_{ij}$ , and average them at the school level, by school membership in 2011. The  $\bar{e}_j$  are the school-level statistics of interest for each grade cohort (studentization of residuals is also inconsequential given these sample sizes). Table 3 shows sample sizes for students and schools, for schools with grade cohort sizes of 30 or more (around 90% of schools and 98% of students). The observed skewness and kurtosis of the 2010 and 2011 score distributions are also shown.

To normalize score distributions, we assign each score the normal deviate corresponding to the percentile rank of the score. Let  $r_x$  be the rank of score  $x$ , where tied values are all assigned their average rank. The normalized score is  $\Phi^{-1}\left(\frac{r_x - 0.5}{n_i}\right)$ , where the Hazen correction is used to avoid untransformable percentile ranks of 0 or 100 (Barnett, 1975). Table 3 also shows the skewness and kurtosis of normalized distributions. Distributions will not be perfectly symmetrical or mesokurtic due to their discreteness.

The first outcome of interest is the difference between  $r_{xy}$  from observed vs. normalized data. As expected, the correlation is higher when score distributions are normalized. The practical significance of this from an interpretive standpoint is interpretable in terms of this statistic alone: one standard deviation unit difference in  $x$  predicts a .64 standard deviation unit difference in  $y$  under observed scores but a .75 standard deviation unit difference under normalized scores. Non-normality generally degrades correlations and predictions (in terms of  $R^2$ , easily calculated from Table 3). As longitudinal analyses increasingly have consequences for individual reporting, including the prediction of future attainment using growth models (e.g., Castellano & Ho, 2013a), conventional predictive models will underperform on predictive performance unless they accommodate the non-normality of these data.

The second outcome of interest is the correlation between the  $\bar{e}_j$  for normalized and observed scale scores,  $r_{\bar{e}_j^{norm}, \bar{e}_j^{obs}}$ . In addition, for illustration, we report percentiles from the distribution of

absolute differences in school percentile rank. Each absolute difference in percentile rank answers the question, how many percentile ranks, in the distribution of school  $\bar{e}_j$ , would a school change, if we switched from observed scores to normalized scores? Table 3 shows that the correlations range from .96 to .99 across different grade cohorts.

Although these correlations seem high, correlations between metrics that use identical baseline data should be expected to be high. Deviance metrics like absolute differences tend to describe practical differences more clearly. Here, Table 3 notes that the median difference in absolute percentile rank ranges from 1.5 to 4.4. The last column shows the 90<sup>th</sup> percentile of the absolute difference in the percentile rank of schools. For these top 10% of schools, as ranked by the absolute difference in percentile ranks between using observed and normalized scores, the absolute difference in percentile rank is greater than 4.2 for the grade 5-6 cohort (a 50<sup>th</sup> percentile school could become a 54<sup>th</sup> percentile school or greater) and greater than 13.6 for the grade 3-4 cohort (a 50<sup>th</sup> percentile school could become a 63<sup>rd</sup> percentile school or greater).

These comparisons are best considered illustrative rather than inferential. They purposefully represent the “blind” application of a simple analysis to data without initial exploratory analysis of data features. Given the skewness and the nonlinearity of the relationship between the score distributions in certain grade cohorts, alternative model specifications will likely be warranted. These options are well known to any student of regression and are therefore not reviewed in depth here, but they include transformations, nonlinear functional forms, robust regression, quantile regression, and nonparametric methods. Among these approaches, statistical approaches like Box-Cox transformations (Box & Cox, 1964) are preferable over straight normalization given the tendency of the normalizing transformation to overfit the sample data.

## Discussion

We have reviewed the features of state test score distributions in terms of skewness, kurtosis, and discreteness, and we have also described the raw-score-to-IRT-scale-score pathology that leads so frequently to non-normal, irregularly discretized distributions. We have also illustrated the kinds of

practical consequences that can arise in selected contexts. Here, we conclude with three general comments reflecting on these results and demonstrations.

First, we selected skewness and kurtosis as convenient indices of the degree of departure from normality because they are well established and generally interpretable. However, these metrics are not universally embraced, and expressions of the magnitude of non-normality are still not as commonly understood as, for example, expressions of the magnitude of effects in meta-analysis (Cohen, 1988; Hedges & Olkin, 1985). It is just as difficult and necessarily contextual to determine whether a .5 standard deviation unit effect is small, medium, or large as it is to determine whether a skewness of .5 is mildly skewed, moderately skewed, or heavily skewed, but there is far more effort toward the former and far less effort toward the latter. We look forward to greater understanding around metrics that attempt to describe the magnitude of non-normality and associated consequences in particular contexts.

Second, nonetheless, skewness and kurtosis seem useful as indices to facilitate study of violating assumptions in many cases. Formally, Yuan, Bentler, and Zhang (2005) provide a nice primer on the known effects of skewness and kurtosis upon inference for the mean and variance in the univariate case, and they generalize from there to discuss implications for multivariate covariance structure analysis. Practically, Castellano & Ho (2013b) compare quantile-regression-based Student Growth Percentiles (SGPs; Betebenner, 2009) to more parametric alternatives across varying levels of skewness and find that SGPs begin to dominate at particular levels of skewness. Theoretically, distributions like the skew-normal (Azzalini & Capitanio, 1999) allow for simulations to study the consequences of violating normal assumptions in terms of skewness in controlled conditions. The concept of skewness and, to a lesser degree, kurtosis, is likely to remain useful in indexing non-normality.

Third, we used Figure 3 to motivate the consideration of discreteness, particularly the common pattern whereby intervals between score points are sparser at high ends of distributions. We observed that these features can be explained as the result of easy tests that generate a negatively skewed raw score distribution, along with a scaling and scoring model that resembles a logistic transformation in its practical consequences. Notably, the rise of the Common Core State Standards in state and federal policy

in the United States (Porter, McMaken, Hwang, & Yang, 2011) is likely to increase the “rigor” of state tests in the near term. This purported rigor will largely arise from changes to content standards, as well as changes to the judgmentally determined cut scores that determine “proficiency.” However, to the extent that rigor manifests in more central average item percentages correct,  $\bar{p}_i \approx 0.5$ , our analysis suggests that more apparently symmetric discrete distributions will result, with smaller percentages at high score points and less spacing between these points. This may have the unintended benefit of ameliorating the problems of individual score reporting, selection into programs, and interpretation of gains that we described in Part 3. More generally, we hope that irregular discreteness, whether or not it is described as a ceiling effect, continues to receive attention as a byproduct of the practical application of IRT scaling methods and as a possible threat to particular uses of test scores.

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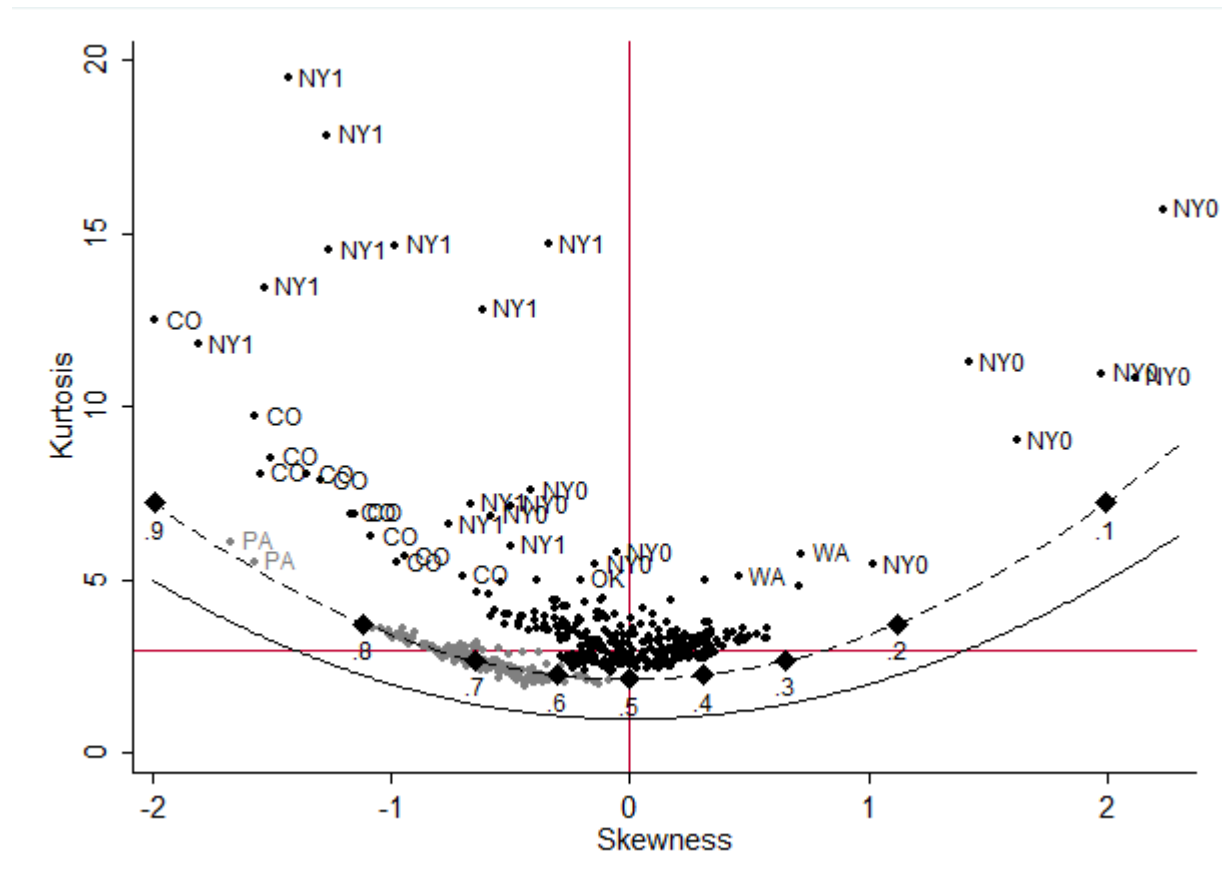


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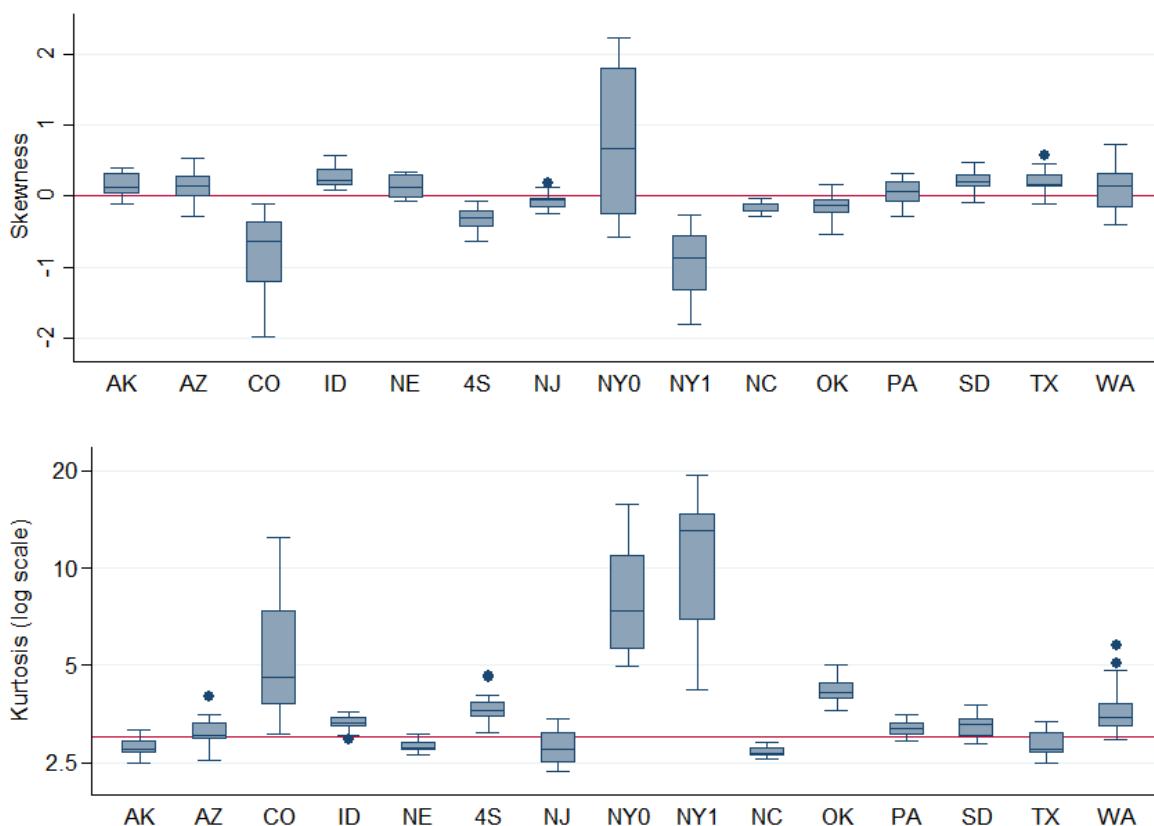
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Figure 1. Skewness and kurtosis of raw score (gray,  $n=174$ ) and scale score (black,  $n=330$ ) distributions from 14 state testing programs, grades 3-8, reading and mathematics, 2010 and 2011.



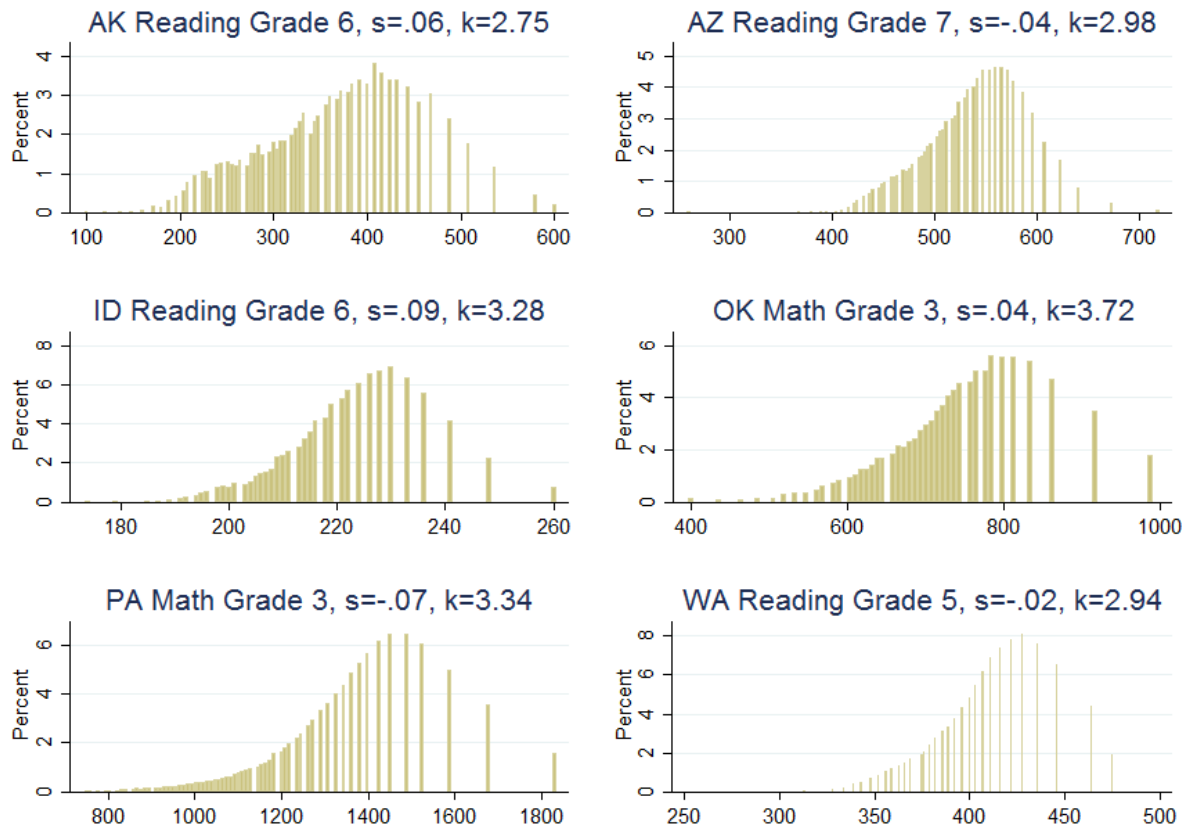
Notes: Distributions with kurtosis  $> 5$  are labeled with their state abbreviations: CO=Colorado; NY0=New York, 2010; NY1=New York, 2011; OK=Oklahoma; PA=Pennsylvania; WA=Washington. The theoretical lower bound of skewness and kurtosis is shown as a solid curve. The skewness and kurtosis of beta-binomial distributions are shown as a dashed line as a function of the average item proportion correct,  $\mu_p$ , under the constraint that the test comprises 50 dichotomously scored items with item difficulties distributed as a beta distribution with parameters  $\alpha$  and  $\beta$  that sum to 4.

Figure 2: Skewness and kurtosis of scale score distributions from 14 state testing programs, grades 3-8, reading and mathematics, 2010 and 2011, shown as boxplots by state abbreviations.



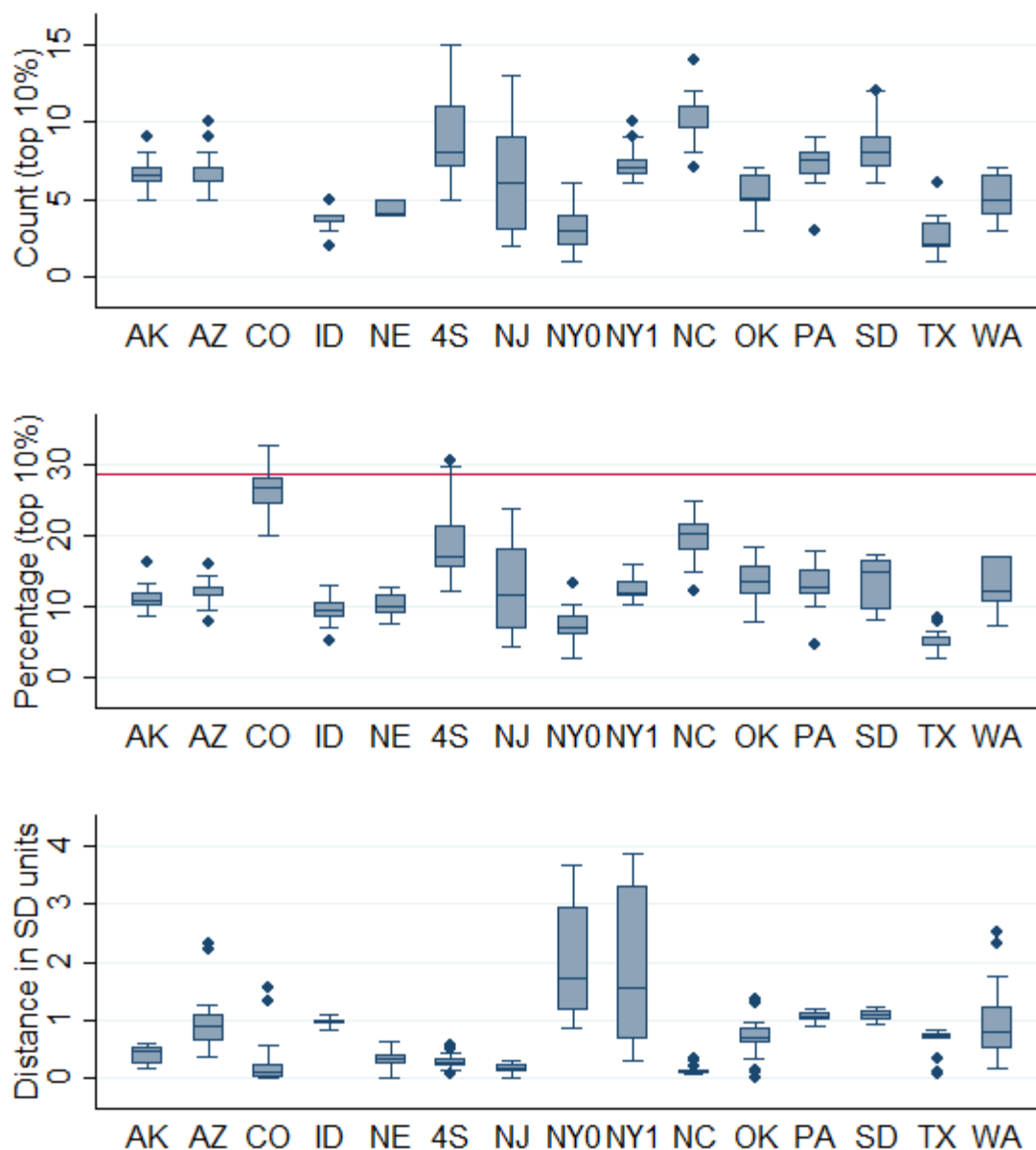
Notes: States are abbreviated: AK=Alaska; AZ=Arizona; CO=Colorado; ID=Idaho; NE=Nebraska; 4S=New England Common Assessment Program (Maine, New Hampshire, Rhode Island, Vermont); NJ=New Jersey; NY0=New York, 2010; NY1=New York, 2011; NC=North Carolina; OK=Oklahoma; PA=Pennsylvania; SD=South Dakota; TX=Texas; WA=Washington.

Figure 3. Six discrete histograms of scale scores selected from a pool of 46 symmetric, mesokurtic distributions with skewness between  $\pm 0.1$  and kurtosis between 2.75 and 3.75. Distributions chosen to illustrate characteristic stretched, high density upper tails in spite of near-zero skewness.



Notes: All distributions are from 2011.  $s$ =Skewness;  $k$ =Kurtosis.

Figure 4. Upper-tail features of scale score distributions from 14 state testing programs, grades 3-8, reading and mathematics, 2010 and 2011.



Notes: Top tile: Count of discrete score points distinguishing among the top 10% of examinees. Middle tile: Percentage of total discrete score points distinguishing among the top 10% of examinees. Bottom tile: Distance from the second highest score point to the highest score point in standard deviation units. States are abbreviated: AK=Alaska; AZ=Arizona; CO=Colorado; ID=Idaho; NE=Nebraska; 4S=New

England Common Assessment Program (Maine, New Hampshire, Rhode Island, Vermont); NJ=New Jersey; NY0=New York, 2010; NY1=New York, 2011; NC=North Carolina; OK=Oklahoma; PA=Pennsylvania; SD=South Dakota; TX=Texas; WA=Washington.



Table 1. Descriptive statistics for scale score and raw score distributions from 14 state testing programs.

State	Test Name	Number of Distributions		Number of Students		Number of Discrete Scale Score Points	
		Scale	Raw	Min	Max	Min	Max
Alaska	Alaska Standards Based Assessment (SBA)	24	24	9237	9737	55	62
Arizona	Arizona Instrument to Measure Standards (AIMS)	24	24	78216	83106	49	64
Colorado	Colorado Student Assessment Program (CSAP)	24	---	57130	62868	466	563
Idaho	Idaho Standards Achievement Tests (ISAT)	24	24	20356	21635	38	44
Nebraska	Nebraska State Accountability (NeSA) Assessment	18	18	20401	21921	36	52
New England	New England Common Assessment Program (NECAP)	24	---	33180	47716	41	54
New Jersey	New Jersey Assessment of Skills and Knowledge (ASK)	24	24	100385	103382	42	64
New York	New York State Testing Program (NYSTP)	24	---	196425	206346	28	67
North Carolina	North Carolina End of Grade Test (EOG)	24	---	104369	115611	48	60
Oklahoma	Oklahoma Core Curriculum Test (OCCT)	24	12	40214	44618	37	43
Pennsylvania	Pennsylvania System of School Assessment (PSSA)	24	24	124535	132906	38	69
South Dakota	South Dakota State Test of Educational Progress (STEP)	24	24	8982	9245	50	80
Texas	Texas Assessment of Knowledge and Skills (TAKS)	24	---	314832	689938	36	93
Washington	Measurements of Student Progress (MSP)	24	---	74726	77324	34	61

Table 2. Median, minimum, and maximum skewness and kurtosis, by scale score and raw score, and by state.

State	Number of Distributions		Skewness						Kurtosis					
			Median		Minimum		Maximum		Median		Minimum		Maximum	
	Scale	Raw	Scale	Raw	Scale	Raw	Scale	Raw	Scale	Raw	Scale	Raw	Scale	Raw
Alaska	24	24	0.132	-0.464	-0.110	-0.679	0.393	-0.342	2.751	2.192	2.476	1.958	3.166	2.560
Arizona	24	24	0.134	-0.659	-0.292	-0.956	0.532	-0.181	3.021	2.591	2.547	2.168	3.990	3.346
Colorado	24	---	-0.642	---	-1.986	---	-0.111	---	4.635	---	3.069	---	12.491	---
Idaho	24	24	0.219	-0.847	0.080	-1.074	0.572	-0.483	3.328	3.114	2.931	2.476	3.594	3.601
Nebraska	18	18	0.129	-0.683	-0.072	-0.904	0.340	-0.523	2.783	2.686	2.639	2.409	3.049	3.301
New England	24	---	-0.310	---	-0.639	---	-0.080	---	3.620	---	3.103	---	4.634	---
New Jersey	24	24	-0.061	-0.481	-0.241	-0.710	0.178	-0.089	2.757	2.503	2.347	2.018	3.407	3.229
New York 2010	12	---	0.664	---	-0.578	---	2.234	---	7.362	---	4.971	---	15.693	---
New York 2011	12	---	-0.870	---	-1.806	---	-0.265	---	13.129	---	4.216	---	19.487	---
North Carolina	24	---	-0.202	---	-0.291	---	-0.032	---	2.669	---	2.571	---	2.881	---
Oklahoma	24	12	-0.127	-0.583	-0.541	-0.948	0.170	-0.286	4.134	2.703	3.606	2.280	5.020	3.593
Pennsylvania	24	24	0.055	-0.673	-0.286	-1.668	0.322	-0.421	3.181	2.666	2.908	2.339	3.525	6.112
South Dakota	24	24	0.194	-0.425	-0.088	-0.780	0.479	-0.175	3.265	2.349	2.865	2.100	3.758	2.847
Texas	24	---	0.160	---	-0.102	---	0.579	---	2.748	---	2.491	---	3.333	---
Washington	24	---	0.146	---	-0.395	---	0.722	---	3.451	---	2.936	---	5.750	---
Med(med), min(min), max(max)			0.129	-0.621	-1.986	-1.668	2.234	-0.089	3.265	2.628	2.347	1.958	19.487	6.112

Table 3. The impact of observed vs. normalized scores on skewness, kurtosis, grade-to-grade correlation, and school percentile rank on a “value added” metric.

Grade Pair	Number of Students	Skewness				Kurtosis				Correlation (2010,2011)		Number of Schools	$r_{\bar{e}_j^{norm}, \bar{e}_j^{obs}}$	Absolute Difference in School Percentile Rank	
		2010		2011		2010		2011		Obs.	Norm.			Median	90 <sup>th</sup> Pctile
		Obs.	Norm.	Obs.	Norm.	Obs.	Norm.	Obs.	Norm.						
3-4	181847	1.17	-0.22	-0.17	-0.01	4.94	2.64	4.02	2.95	0.64	0.75	2177	0.956	4.41	13.62
4-5	185131	0.45	-0.06	-0.58	-0.01	4.88	2.85	6.04	2.94	0.78	0.81	2129	0.982	2.60	7.70
5-6	181912	0.04	-0.06	-0.32	-0.02	5.13	2.85	5.59	2.91	0.75	0.79	1465	0.994	1.47	4.22
6-7	183514	-0.40	-0.03	-0.46	-0.01	6.64	2.86	6.82	2.93	0.78	0.82	1250	0.989	1.95	5.56
7-8	184698	-0.29	-0.02	-1.62	0.00	7.08	2.91	11.86	2.92	0.76	0.83	1233	0.962	3.51	9.74

Notes. Skewness and kurtosis of observed (Obs.) and normalized (Norm.) scores are shown. Normalization does not result in skewness=0 or kurtosis=3 due to the discreteness of the score distributions. The Correlation(2010,2011) values show the grade-to-grade correlation for observed and normalized scores. The  $r_{\bar{e}_j^{norm}, \bar{e}_j^{obs}}$  correlation is that between the school value added on the observed scale and the school value added on the normalized scale.